
ANALYSIS

Edited by
Margaret Macdonald

with the advice of

A. J. Ayer	A. E. Duncan-Jones
R. B. Braithwaite	C. A. Mace
Herbert Dingle	A. M. MacIver
H. H. Price	

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NOTE ON PROSPECTUS

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SYNONYMITY AND LOGICAL EQUIVALENCE

By ARTHUR PAP

AN adequate explication of synonymity is important for philosophical analysis, just as an adequate explication of entailment is vital for the deductive logician. The logician needs reliable criteria to determine whether one proposition is entailed by another. The philosophical analyst needs a criterion to determine whether two given expressions are synonymous, since the problem of analysis might be equally well described as the problem of finding for a given expression a synonym which is, in a sense difficult to analyze, more *articulate*. I should point out, however, that in describing the problems of philosophical analysis in the 'formal mode of speech', I do not commit myself to a rejection of Moore's conception of analysis as being concerned with concepts rather than verbal expressions: for there is a sense in which the statement "'A' is synonymous with 'B' means no more nor less than "the concept *being an A* is identical with the concept *being a B*". This is the sense in which the above statement of synonymity could not be refuted or verified by reference to facts of linguistic usage, but means (putting it somewhat loosely): the meaning which happens to be expressed by the symbol 'A' is identical with the meaning which happens to be expressed by the symbol 'B'. To illustrate: if the statement "'father' is synonymous with 'male parent'" expresses an *analysis* in Moore's sense, then it cannot be invalidated by any future change of linguistic rules, e.g. by 'father' coming to be used in the sense in which 'parent' is now used. All that need be admitted as the result of such an

event is that our verbal expression of the intended analysis is no longer correct.¹

One further introductory observation should be made. It does seem *circular* to give an explication of the concept of synonymy (or, more accurately, of that one of the various meanings of the word 'synonymous' which is relevant to philosophical analysis), if such explicative statements themselves assert synonymy: it is circular to use a concept as a means of analyzing itself. This, however, is the same sort of apparent circularity as is involved in the attempt to define, say, 'logical equivalence'. Is not the concept of logical equivalence used as a means of definition when "p is logically equivalent to q = df "p is deducible from q, and q is deducible from p"?² Not quite; for while the logical equivalence of *definiens* and *definiendum* follows from the definition, the definition does not assert it. Similarly, an explication (analysis) might always be so formulated that while a statement of synonymy may be derived from it, no such statement is asserted by it. "'A' means B" does not mention the expression 'B', hence it does not assert a relation between the expressions 'A' and 'B'; but it gives rise, in a perfectly intelligible sense, to a rule of substitution. Another way in which the objection from circularity could have been rebutted so as to satisfy formal logicians, is to say that the terms in which an analysis is formulated must belong to a higher language than the terms whose meanings are analyzed, that the second-order term 'synonymous'₂ is used in order to define the first-order term 'synonymous'₁. But I do not want to rely too much on this kind of argument (familiar from Russell's theory of types) since it might invite the objection that what the philosopher wishes to analyze are non-linguistic concepts, not verbal expressions belonging to a specific language-system.

In order to spare the reader a disappointment, I shall admit at the outset that I have no satisfactory analysis to offer of that sense of 'synonymous' in which, say, 'father' is synonymous with 'male parent', which differs from that in which 'father' is synonymous with 'Vater'. I am not at all clear about the

¹ Some may say that in using 'meaning' as a substantive one has already succumbed to Platonism; i.e. one treats the verb 'to mean' as though it were transitive and invents abstract objects that are meant, comparable to women as concrete objects that are loved. There is no space in this paper for a discussion of this old issue. I confine myself to the remark that there is no more force in this accusation than there would be in the accusation that one commits oneself to the belief that there must exist songs to be sung before singing can take place, just because one uses the substantive 'song' in order to be able to use 'to sing' transitively.

² To avoid awkward quotation devices, I am here using the letters 'p' and 'q' as variable names of sentences, i.e. as variables whose values are names of sentences, not, as is customary in the sentential calculus, sentences.

precise difference between a translation and an analysis.¹ All I claim to do is to reveal a defect in recent proposals of explicata for the concept of synonymy, viz. those of Lewis (*An Analysis of Knowledge and Valuation*, book I) and Carnap (*Meaning and Necessity*). Both Lewis and Carnap want to define synonymy so that it will turn out to be a stronger relation than logical equivalence. Evidently not all logically equivalent sentences are synonymous, or all analytic, including all self-contradictory sentences, would be synonymous. Now, both Lewis and Carnap use the concept of *intension* as their basic instrument of analysis. There are, to be sure, differences in their use of this term. Thus Lewis defines the intension of a predicate *explicitly* as the class of predicates entailed by applications of the given predicate. Carnap defines 'intension' only in context, or what he explicitly defines is only the phrase 'same intension': two predicates 'P' and 'Q' have the same intension if the equivalence " $(x)[Px \equiv Qx]$ " is analytic (L-true). Carnap thus leaves the question open whether intensions are to be regarded as classes of logically equivalent expressions or as non-linguistic *designata*; while Lewis treats intensions definitely as classes of verbal expressions and uses a special term ('signification') for non-linguistic *designata*. But in this context such differences are negligible and the similarity of their approaches is more important. 'Intension' is defined (whether explicitly or in context) in terms of logical equivalence, and thus indirectly 'synonymy' is defined in terms of logical equivalence. I propose to show, now, that this procedure is fatally circular.

Let me begin with Lewis's concept of 'equivalence in analytic meaning'. Sameness of intension is a necessary but insufficient condition for two expressions to be equivalent in analytic meaning. If they are both complex, then their corresponding constituents must have the same intension (an example used by Lewis is the pair of complex expressions 'circular hole' and 'round excision'). This conception helps Lewis, indeed, to clarify the sense in which terms of universal intension (like 'round square') as well as terms of zero intension (like 'square or not square') fail to have the same meaning: they are usually built

¹ Moore has suggested that an analysis differs from a translation because the correctness of a translation may be known without knowing the meaning of either one of the inter-translated expressions, while the correctness of an analysis could not be known in such circumstances. Presumably the correctness of a translation is knowable without semantic knowledge in the same sense in which I can know that the people who call themselves my parents are my parents, without having witnessed any events prior to my birth: I accept the proposition on authority. But why could I not similarly 'know' that the various analyses contained in a dictionary are correct, viz. by inference from the semantic erudition of the dictionary maker?

up with the help of descriptive terms in such a way that corresponding constituents are far from being logically equivalent. But what if one of the two logically equivalent expressions is elementary, as in 'square = equilateral rectangle'? Lewis's answer is that, if so, logical equivalence is also a sufficient condition for synonymy. Now, the above is an example of an *explicative statement*, and consistently with the conditions laid down for equivalence in analytic meaning, Lewis regards explicative statements as a species of *analytic* statements. It would seem to follow that in order to determine whether the explicative statement "a square is an equilateral rectangle" is true, we have to determine whether it expresses a logical equivalence, an analytic truth; for to say that it is a correct explication is to say that 'square' is synonymous with 'equilateral rectangle'. If Lewis were asked what he means by an analytic statement, he would presumably reply, a statement which is derivable from principles of logic with the help of definitions. But if definitions are arbitrary linguistic conventions, could not any statement become analytic, e.g. "all swans are white" relatively to the definition of 'swan' as 'white bird capable of swimming and inspiring poets'? Lewis, perhaps justifiably, repudiates this 'conventionalist' interpretation of analytic truth by pointing out that those definitions, though capable of functioning as syntactic rules, primarily express knowledge of identity of meanings, i.e. are explicative statements. Now, however, the vicious circle looms larger. To show that "a square is an equilateral rectangle" is analytic, it must presumably be derived by substitution of descriptive terms from the principle of identity " $(x) (P) [Px \equiv Px]$ ". Now which definition is to lead to the substitution-instance 'a square is a square' if not the very definition "square = equilateral rectangle" which is to be proved as adequately equating identical meanings? To propose demonstration of logical equivalence as the test of synonymy seems, therefore, to put the cart before the horse.

A similar, though better hidden, circularity seems to me involved in Carnap's explication of the concept of synonymy in terms of what he calls 'intensional isomorphism'. Carnap stipulates L-equivalence of sentences as the symmetrical and transitive relation by which propositions are logically constructed, i.e. two sentences are to designate the same proposition if they are logically equivalent. Obviously, according to this usage of 'proposition', two sentences may express the same proposition without being synonymous. First, any two L-true sentences, and any two L-false sentences, are logically equiva-

lent : Carnap consequently speaks of *the* L-true, and *the* L-false, proposition. Secondly, if an explicative statement, like "to be a father is to be a male parent", is true, it expresses a logical equivalence; but is itself equivalent to the trivial identity obtained from replacing the *analysans* by the *analysandum* ("to be a father is to be a father"). Carnap should say, in accordance with his convention for the use of the word 'proposition', that "to be a father is to be a father" expresses the same proposition as "to be a father is to be a male parent" (which has been called the 'paradox of analysis'); so the task remains to explicate the sense in which the two sentences, though logically equivalent, fail to be synonymous. Now, Carnap's concept of intensional isomorphism corresponds to Lewis's concept of equivalence in analytic meaning in so far as two complex expressions, like ' $7 > 5$ ' and ' $\text{Gr}(7, 5)$ ' are said to have the same intensional structure if corresponding constituents, like ' $>$ ' and ' Gr ', down to the elementary constituents, are L-equivalent.¹ There is an important difference, however, since Carnap does not allow an elementary expression to be intensionally isomorphic with a compound expression (one made up of sub-designators, like 'male parent'). Thus Lewis's explication of synonymy leads to the conclusion that 'father' is synonymous with 'male parent' (provided the explicative statement "to be a father is to be a male parent" is correct), while Carnap's explication entails the very contradictory. What we need, however, is an explication of that sense of 'synonymous' in which a correct analysis can be said to express synonymy without being either trivial ("a father is a father") or a mere dictionary translation ('father' = 'Vater').

So far I have merely shown that Carnap's concept of intensional isomorphism fails to provide a criterion of a correct analysis; which, indeed, would be a relevant criticism only if Carnap had claimed that it did. I proceed to explain why Carnap's explication of synonymy lands one in a fruitless circle once one attempts to use it in order to test synonymy. Take the above illustration (used by Carnap) of the pair of synonyms ' $7 > 5$ ' and ' $\text{Gr}(7, 5)$ '. In order to show that the matrices ' $\dots > \dots$ ' and ' $\text{Gr}(\dots, \dots)$ ' are L-equivalent, I have to show that the identity of their truth-values (or, rather, of the truth-values of the schematized statements) *follows from the semantic rules*. Indeed, this can easily be shown by referring to the

¹ Carnap widens the use of 'L-equivalent' in such a way that this semantical term is predicable not only of sentences but of other designative expressions, like predicates and descriptions, as well.

semantic rules ' $\dots > \dots$ ' means that ' \dots is greater than \dots ' and ' $\text{Gr}(\dots, \dots)$ ' means that ' \dots is greater than \dots '. Now, if all this analysis takes place within a language *system* as envisaged by Carnap, then these semantic rules are simply conventions and there can be no question of their cognitive justification. However, if the test of synonymy is to be applied to expressions in a natural language, then the whole procedure is circular since the semantic rules would themselves be statements of synonymy. That is, in order to know that ' $\dots > \dots$ ' means that ' \dots is greater than \dots ', I would have to know that ' $\dots > \dots$ ' is *synonymous with* ' \dots is greater than \dots ', continued reference to semantic rules would lead to an infinite regress. Since Carnap is forging tools for the semantic analysis of language *systems*, this criticism cannot be brought against the utility of his analysis for the purposes of pure semantics. But it does show that the analysis of synonymy in terms of a concept of logical equivalence defined with reference to language systems fails to help those who want to apply semantic analysis to natural languages.

If logical equivalence is unsuitable as an *explicatum* of synonymy, what possibilities remain? The old Leibnizian *salva veritate* test ("two terms are synonymous if one can be substituted for the other in any statement without changing the truth-value of the statement") is obviously unsatisfactory since extensional equivalence is no sufficient condition for synonymy. Possibly an explication of this concept must be given within pragmatics, in terms of verification; and since natural languages do not contain clear-cut semantic rules or definitions, the same may be true of logical equivalence. Perhaps it is satisfactory to say, with Ayer, that "'A' and 'B' are synonyms if and only if any conceivable verification of 'A' includes verification of 'B'", and conversely. At least this will be fully satisfactory if the relevant sense of 'conceivable' can be clarified. There is a perfectly intelligible sense of the word 'conceivable' in which two predicates might fail to be synonymous although all conceivable tests of applicability coincide, for the simple reason that we cannot conceive of a separate existence of the connoted properties. Thus I cannot conceive of a situation in which the predicate 'shape' is applicable but the predicate 'size' is not applicable, or conversely. It may be replied that if the two predicates fail to be synonymous it is because there are conceivable situations where 'same size' is applicable and 'same shape' is not applicable, or where 'same shape' is applicable but not 'same size'. But then what may be a superficial interpretation of the verifiability theory of synonymy leads to a

paradoxical consequence : ' same size ' and ' same shape ' fail to be synonyms, even though ' same ' is evidently used univocally and ' size ' is synonymous with ' shape ' by the verification test. It may not be unfair to say, then, that the verifiability theory of synonymy needs to be more subtly formulated, before it can be used to solve the problem of explicating the concept in terms of which all explications are expressed.

MORAL SUBJECTIVISM—DR. EWING'S METHOD

By H. B. ACTON

IN my criticism (ANALYSIS, Vol. 9, No. 1) of Dr. Ewing's *The Definition of Good* I dealt with the details of the six arguments against Personal Subjectivism he gave on pp. 3-7 of that book, and I also suggested that there was something wrong with his method of refuting that view, in that he was too ready with claims to ' see ' the truth of propositions held to be incompatible with the theory he was refuting. Anyone who is interested can now compare Dr. Ewing's detailed answers (ANALYSIS, Vol. 9, No. 2) with my criticisms, and I do not propose embarking on any further public discussion of them. He has also, however, defended himself against my more general criticisms of his method, and I think there may be something to be gained by further discussion of this topic. In his reply Dr. Ewing says : " What we ' mean ' is what we intend to assert " (p. 17), and also : " the meaning of something just is what we intend to assert by it " (p. 21) In *The Definition of Good*, too, he had written : " ' What I mean ' is, in the case of propositions, ' what I intend to assert ' " (p. 44). I think that he considers that this principle carries the conclusion that the way to find out what we mean is by introspection of our intentions, and that such introspection, though not infallible, is pretty reliable. This, if true, is important for philosophical method, and merits full discussion. In the present note I can touch on only two points.

(a) That I cannot accept as correct a suggested analysis that does not seem correct to me is a tautology. But that is not the same thing as to say that the way to discover whether a suggested analysis is correct is to introspect my intentions. For surely it is possible that I am peculiar in my intentions and use words in ways that other people do not, just as I might have some idiosyncrasy of pronunciation. When philosophers attempt to

formulate the meaning of an expression in the sense of its analysis they are not interested in personal peculiarities of meaning, but in some generally accepted meaning, not with what one man *wishes* to assert, but with what people generally *succeed* in asserting. This, it seems to me, is in no way affected by the tautology that I cannot accept as a correct analysis what does not seem to me to be a correct analysis.

(b) Even if we grant, therefore, that "what we 'mean' is what we 'intend to assert'", the emphasis must be upon the word 'we', and to discover what *we* intend to assert it is not sufficient for me to introspect in order to discover what *I* intend to assert (if indeed that is how to find out my intentions). Dr. Ewing in effect admits this when he thinks it very unlikely that his opponents' minds are fundamentally different in structure from his (p. 18). How, then, can I discover what other people intend to assert in order to compare it with what I intend to assert? I can ask them. They then explain to me what it is they intend to assert. But do they intend to assert the same thing by their explaining words as I should intend to assert if I used them? I can ask again. But to this there is no end and I shall be led on *in infinitum*. Perhaps, then, I am to say that I know that their intentions are the same as mine. But how can I come to know this? Not by direct acquaintance with their intentions, but by observation of how they use the words I am enquiring about. Thus it seems to me that in spite of what Dr. Ewing has said we do assure ourselves of the meanings of words not by introspection but by observing how they are used. The fact that, as he suggests, we feel towards a correct analysis: "Ah, this is what I really meant all along, only I did not put it so well", shows only that we knew how the expression was used by ourselves and others, but had not, until that moment, seen a satisfactory way of saying how it was used.

IF'S and AND'S

By P. T. GEACH

MY aim in this note is to show that the interpretation of truth-tables and propositional calculi needs a good deal more care than certain logicians seem to think. I shall use the following abbreviations: ' $\neg p$ ' for 'the contradictory of the sentence p ' (*i.e.*: 'the sentence that is true if and only if p is false, and is false if and only if p is true'); ' $p \cdot q$ ' for 'the conjunction, simultaneous assertion, of the sentences p and q ';

' pIq ' for 'the hypothetical with p as antecedent and q as consequent'.

(1) I shall first consider a misunderstanding in Lewis and Langford's *Symbolic Logic* (p. 227). They begin by saying: 'Whatever more it may be, the matrix method at least is a kind of game which we play with recognisable marks, according to certain rules'. All that this really tells us is that in doing logic we write down definite symbols in an orderly way; which throws no light at all upon the nature of logic. But the natural suggestion of the word 'game' is that logic is something like noughts-and-crosses; I do not know if this was intended by the authors.

Their next remarks have reference to the following extract from the truth table for pIq (which is given on p. 203, with ' $p \supset q$ ' instead of ' pIq '):

p	q	pIq
1	1	1
1	0	0

They rightly assert that such tables need not have "any 'logical' significance"; p and q may be 'any kind of things'. What is required, on their view is that in some game or other there should be "an operation or move, pIq , which according to the rules can be taken, when p has the property A, only if q also has the property A"; in that case the table will tell us that if p has A, and pIq is an allowable move, then q must have A. The logical interpretation will then consist in taking the property A, expressed by the figure '1', to be truth, and the property expressed by the figure '0' to be falsehood, and reading ' pIq ' in the way I explained above.

But there is an inconsistency here in the interpretation of the figure '1', if p and q may be 'any kind of things'. As regards p and q , the figure '1' is taken to stand for the property A; but as regards pIq it is taken to stand for the property of being an allowable move in a certain game. This is inconsistent, unless A is the property of being an allowable move; and of course that is not necessary. We see this clearly if we take a simple non-logical interpretation of the table. Let the figures '1' and '0' connote the presence and the absence of an hereditary property A, and let ' pIq ' mean 'offspring by p out of q '. Then the table will have no reference to moves in any game; it will tell us that the trait A is present in the offspring, when it is present in the sire, if and only if it is also present in the dam.

We are indeed at liberty to take the figures '1' and '0' to mean that moves in a game are respectively allowed and for-

bidden ; but in that case p and q cannot be 'any kind of things', but must be moves in the game, like pIq . The table will then be equivalent to the following sentence : "if p is an allowable move, then q is an allowable move if and only if pIq is also". And here the sign 'I' does not belong to the terminology of any special game, like 'Kt' in chess ; it expresses what we may fittingly call a logical relation of the move pIq to the moves p and q ; so that the significance of ' pIq ' is after all 'logical'.

I think the source of these confusions is the authors' habitual carelessness over the use of quotation marks. Whatever sort of things p and q may be, the printed sign ' pIq ' is a move in 'a kind of game which we play with recognisable marks'—if you choose to abuse the word 'game' that way. But it is not the case that, however the table is read, pIq must be taken to be a move in a game ; for pIq need not be a printed sign, as ' pIq ' is ; in the non-logical interpretation I gave above, pIq was an animal.

(2) In some many-valued logics, there is a truth-value such that if p has it, then $\neg p$ has it ; I shall use 'X' as a sign for this truth-value. Lewis and Langford suggest as interpretations of ' p has the truth-value X' the ordinary English expressions ' p is doubtful' and ' p is as probable as not' (*op. cit.* pp. 223, 230). Similarly, some Polish logicians have ascribed the truth-value X to assertions about future contingencies, e.g. the weather ; the contradictories 'it will rain to-morrow' and 'it will not rain to-morrow' have on their view the same truth-value.

It is easy to show that this sort of interpretation will not do. If p and q have the same truth-value, then $p.q$ has the same truth-value as $p.p$. This depends merely on the fact that $p.q$ is a truth-function of p and q ; it does not depend on our choice of a truth-table for $p.q$, nor yet on whether our logic is two-valued or many-valued. Again, $p.p$ must have the same truth-value as p has. This is so because ' $p.p$ ' is short for 'the simultaneous assertion of p and p ' ; a truth-table that did not always give $p.p$ the same truth-value as p would simply not be defining the right truth-function. It follows that if p and q both have the same truth-value, then $p.q$ must likewise have that truth-value ; so if p has the truth-value X, $\neg p$ also has the truth-value X, and $p.\neg p$ likewise has this truth-value. Thus if we admit the truth-value X, the law of contradiction is not a tautology.

This shows no inconsistency in many-valued logics ; their consistency has been fully proved. Nor does it show the inapplicability of the idea of contradictories in many-valued logics. The contradictory of p is a sentence that is true if and only if

p is false, and false if and only if p is true; in three-valued logic there is just one matrix that defines such a truth-function, viz.:—

p	T	X	F
$\neg p$	F	X	T

But what does follow is that the suggested interpretations are ruled out. In ordinary English we may truly say 'it will rain to-morrow as likely as not' or 'it is doubtful if it will rain to-morrow'. But this cannot be taken to imply that 'it will rain to-morrow' has the truth-value X; for in that case 'it will both rain and not rain to-morrow' would have the truth-value X; and clearly its truth-value is in fact falsehood.

(3) According to Lewis and Langford (*op. cit.* p. 228), any of the following four truth-tables defines an implication adequate for the *modus ponens*—"if p is true, and pIq is true, then q is true":—

p	q	(i)	(ii)	(iii)	(iv)
T	T	T	T	T	T
T	F	F	F	F	F
F	T	F	T	F	T
F	F	F	F	T	T

We see at once, however, that the 'implications' (i) and (ii) are hopeless claimants for the title. With either truth-table, pIq would be assertible only if q were so; hence there would be no tautologous 'implications', not even pIp ; and the formula of the *modus ponens*, $pI((pIq)Iq)$, would hold only if q held. The authors recognise in a footnote that some of their 'implications' have this property; but they do not see that this makes the term inappropriate. (iii) and (iv) agree far better with our natural idea of implication; we readily verify that with either truth-table $pI((pIq)Iq)$ is a tautology. With (iii), however, $(pIq)I(qIp)$ is also a tautology; and this does not accord with the logic of the *modus ponens*, in which it is notoriously a fallacy to 'affirm the consequent'. Thus only *one* truth-function in two-valued logic is satisfactory for the *modus ponens*; viz (iv), ordinary material implication. I could likewise show that in three-valued logic the satisfactory implications are far fewer than the 2,916 that the authors say there are.

Similar criticisms apply to Reichenbach's three-valued logic in *Philosophic Foundations of Quantum Mechanics*. He defines a 'quasi-implication' by the following truth-table; I use my sign 'X' instead of his 'I' to stand for the third truth-value—that of

sentences which are neither true nor false but 'indeterminate'. (*Op. cit.* pp. 151-2).

p	T	T	T	X	X	X	F	F	F
q	T	X	F	T	X	F	T	X	F
$p \rightarrow q$	T	X	F	X	X	X	X	X	X

This truth-function has some, but not all, of the properties we expect in an implication; we see, in fact, that with this truth-table $p \rightarrow q$ is assertible only if q is, and that $p \rightarrow p$ is not a tautology, but holds only if p holds; that is why Reichenbach calls this 'quasi-implication'. So far, there is nothing to quarrel over; but we can tell in advance, without having to know quantum theory, that quasi-implication is bound to be scientifically useless. We can know that $p \rightarrow q$, with this truth-table, holds, only when we already know that q holds; and why should we trouble in that case to assert q conditionally upon p , when we can assert q unconditionally? The only hypotheticals useful in science are those that we can know to hold *without* first establishing the truth of the consequent.

Our expectation is confirmed when we see for what sort of hypotheticals Reichenbach proposes this truth-table; viz for hypotheticals of the form 'if a measurement m_i is made, the indicator will show the value q_i ' (*op. cit.* pp. 166-8). It seems doubtful, for various reasons, whether this sort of 'if....then....' is a truth-function of any sort. But Reichenbach's matrix certainly will not do; for, with that, the above assertion could be made only when we had verified that the indicator *did* show the value q_i , and would thus be scientifically useless. Moreover, we should reach the absurd result that 'if this measurement is made, then it is made' is not tautologous, but holds good only if the measurement *is* made. Even a hypothetical that is not a truth-function at all is tautologous when its antecedent and consequent are identical.

THE LOGICAL STATUS OF PSYCHO-ANALYSIS

By HERBERT DINGLE

MR. S. TOULMIN, in his article under this title in *ANALYSIS* for December 1948,¹ has adopted, I think, an unfortunate point of view, from which the chief significance of psycho-analysis is invisible and minor aspects loom large. He considers that the difficulty with such questions as "What *kind* of thing is the 'unconscious mind'?" arises from "thinking of psycho-analysis too much on the analogy of the natural sciences", and he then attempts to show how 'psycho-analytic explanations' differ from, and are related to, three types of 'explanation' which are customary in other departments of thought, including presumably the natural sciences. I want to suggest that 'psycho-analytic explanations' are essentially the same as physical explanations, and that the answer to the question, "What kind of thing is the 'unconscious mind'?" is "It is precisely the same kind of thing as the atom or ultra-violet light".

Psycho-analysis is a scientific study of psychology. It may be good or bad science, but it is undoubtedly authentic in character and method. In the more familiar sciences the procedure is first to isolate the phenomena of interest and then to attempt to correlate them by defining certain concepts to which are given properties appropriate to the task in hand. Thus, in the science of optics we first isolate certain visual phenomena and then form a concept of 'light', to which we assign such properties as will enable us to relate together in a rational system as many of the phenomena as we can. Similarly, in the science of chemistry we isolate the phenomena of chemical reactions and then form the concepts of atoms and molecules to which we assign such properties as will enable us to make a similar rational system in that field. In the science of mechanics we isolate the phenomena of mechanical motion and form the concepts of space, time and mass (the last-named being dispensed with in relativity mechanics) to which we again assign the properties necessary to give us a rational system. The method is essentially the same in the other sciences.

A scientific study of psychology, then, would consist of, first, the isolation of the relevant phenomena, and then the creation of the appropriate concepts for their correlation. The phenomena are, broadly speaking, the observable behaviour peculiar to human beings, and certain experiences such as emotions, passions,

¹ *Analysis*, Vol. 9, No. 2, pp. 23-29.

dreams, etc., not normally dealt with in other sciences, and the concepts would be such as to afford the most comprehensive correlation of them. Let us call one such concept—that most suitable for the correlation of the behaviour of an individual, say—the *psychogen*, merely as a label. Then the task of the scientific psychologist is to assign such properties to the psychogen—to *define* it, in a word—as will enable it to fulfil its function. The unconscious is, in fact, just such a concept as this, and we may therefore write, the psychogen=the unconscious.

I think the chief difficulty in recognising the normal scientific character of psycho-analysis comes from the choice of the word 'unconscious' for the psychogen. It suggests that the word stands for one of the phenomena to be correlated rather than a logical construct useful for correlation. Not only does the word 'conscious' appear as part of it, but also the 'unconscious' is conceived as running continuously through the pre-conscious into the conscious, and the present description of its structure is in terms of wishes, impulses, etc., which are familiar conscious phenomena. It should be remembered, however, that it is a general characteristic of scientific concepts to start their career as imaginary picturable phenomena, and their development consists of a progressive change towards pure concepts without picturable qualities. The earliest concept of light, for instance, was that of a feeler put out by the eye. This changed successively from small material particles, elastic waves in a material medium, electro-magnetic waves in a semi-material medium, to something, indescribable in terms of sensations, travelling along geodesics in a 'space-time' satisfying certain equations. The atom was at first an incompressible material sphere, which changed through a solar system to a probability distribution of a mathematical quantity. And so on. The atom ('that which cannot be cut'), like the unconscious, was given a name representing a negative aspect of a sensible quality, but since divisibility does not seem so comprehensively characteristic of material phenomena as consciousness does of mental phenomena, there is less difficulty in recognising the atom as a concept than there is with the unconscious. Again, the same continuity that is postulated between the conscious and the unconscious is postulated also between gross bodies and atoms; go on cutting a piece of matter, and a time comes when you arrive at single atoms. I am not concerned here to elucidate this paradox. I want only to point out that it characterises ordinary physical science as well as psycho-analysis.

Another aspect of Mr. Toulmin's confusion of phenomena

with explanations is worth pointing out, namely, his view of psycho-analytic 'cures', which he seems to regard as indicating a distinction between psycho-analytic and physical explanations. "If a fully-fledged analytic explanation is not part of a successful cure", he writes, "we do not regard it as a 'correct' explanation; therapeutic failure is as fatal to an explanation in psycho-analysis as predictive failure is to an explanation in physics". Now first of all we must leave out of account the fact that the phenomenon called a 'cure' has a curative aspect, because that is quite accidental. It is a datum of observation that, for instance, the recalling of certain memories results in the cessation of certain mental experiences. The fact that the patient is pleased when this happens is no more relevant to the scientific status of psycho-analysis than the fact that he is pleased when tea dissolves sugar is relevant to the theory of solution. A 'fully-fledged analytic explanation' might in principle just as validly predict that a certain procedure would turn a normal person into a neurotic as that a different procedure would have the reverse effect. That being so, it is clear that a so-called 'cure' is not an explanation, or part of one, and does not include an explanation; it is one of the phenomena to be (or which has been) explained.

The reason why an abnormally large proportion of the phenomena with which psycho-analysis is concerned are associated with cures is clear enough. Opportunities for experiment are much more restricted than in the physical, or even the biological sciences and are for humanitarian reasons largely confined to attempts to remove distressing symptoms. In physics there are almost no such limitations; even the atomic bomb could be tried out in the open spaces of America. In physiology we have still wide freedom because most of the chemistry of the body can be paralleled in laboratory experiments. However, here there are, I believe, some processes that require experiments on the living body itself; for instance, I do not think (I write without knowledge) that the fact that exercise can turn flabby muscles into tough ones would have been known without trial, and the fact that most people prefer tough muscles to flabby ones allows of these experiments being made, whereas an experiment to turn a healthy liver into a diseased one would be permitted only on animals. With most mental phenomena even animals do not provide a satisfactory field for experiment, so the psycho-analyst is almost entirely restricted to attempted cures for his experiments and to their results (just as informative whether they succeed or fail) for his data of observation. But that does not make his explanations a part of a cure, any more than the corres-

ponding effect in physiology makes a theory of muscle behaviour a part of physical training.

I think that the outstanding significance of psycho-analysis for the philosophy of science, and for philosophy in general, is the implication which it carries that there is no fundamental difference between sense-data and the various wishes, fears, exultations, etc., that psycho-analysis deals with. All experiences, of whatever kind, are now shown to be amenable to the same kind of scientific treatment, *i.e.* to correlation in terms of concepts defined for the purpose. This conclusion does not depend on any assumption that the present hypotheses and 'explanations' are valid; they are as likely to be right (*i.e.* lasting) as the early theories of electricity. But the fact that they are successful in correlating certain mental phenomena previously thought to be outside scientific treatment altogether, and that they do so in terms of purely *ad hoc* concepts independent of recognised physical concepts, places emotions and even the content of dreams and hallucinations on the same footing as sights and sounds; all are data of experience susceptible of rational correlation with one another, and the task of science or philosophy is to correlate them as completely as possible.

